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ABSTRACT

This paper explores the dynamic nature of the power relationship within social research, concentrating on the production and reproduction of a text of the field and focusing on the processes of recording, writing, and reading. It draws on ethnographic research conducted in a United Kingdom office of an international firm of chartered accountants and focuses on the learning experiences of a cohort of recent university graduates in their first year of working for the firm. Fieldwork over the course of a year consisted of observation and interviews. The firm, as the researcher's field, became composed of what the researcher chose to record and how she chose to record it. The powerful research setting could make the researcher feel victimized and powerless through criticism and exclusivity of the study population, yet the researcher retained power as the writer reporting that environment. Such power is not fixed or permanent and is subject to reduction by the research population at a later stage. This study of the powerless in a powerful organization demonstrates that textual representations transform social actions and events into narrative, which in turn shapes and gives consequence to the details of observed life. By placing observations into recognizable textual formats, the ethnographer can make the social work readable. (Contains 56 references.) (SLD)

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The Power of Accounts: Ethnographic Research in a Professional Educational Setting

Amanda Coffey

Introduction

This paper explores the dynamic nature of the power relationship within social research. It concentrates on the production and reproduction of a text of the field. A primary focus is therefore placed on the processes of recording, writing and reading (Sanjek 1990, Jackson 1990, Atkinson 1990, Richardson, 1990, Wolcott 1990, Atkinson, 1992, Wolf 1992) and on the authorial status of the ethnographer (Barthes 1982, Geertz 1988, Hastrup 1992). The paper demonstrates that even where the researcher is entering into a 'powerful' research setting, the notion that they are powerless is rather a simplistic one. Power and authority in the context of social research are, at best, ambiguous. Discourses of power and authority are complex aspects of the research experience. That either the researcher or the researched constitute the powerful is questionable. I argue that it is possible for both to negotiate space within the research enterprise.

Studying Up and Writing Down

The tendency of social researchers to 'study down' as opposed to 'studying up' was brought to our attention in the 1970s (Nader 1974, Bell 1978), and has continued to remain a source of debate within sociology. The argument that social research favours the underdog and 'gives voice' to under-privileged and powerless groups in society, was an argument pertinent and valid at the time. There has been a diversification of research subjects and research settings chosen for sociological investigation since the 1970s. In particular there is now a strong literature on the professions (see Atkinson 1983, Dingwall and Lewis 1983, Atkinson and Delamont 1985) and on elite members and their power over knowledge within organizational settings (for example; Freidson 1986, Bryman 1988). The 'sociology of education' has contributed to this trend. The education of professional groups is now reasonably well documented, particularly that of the liberal professions (e.g. Bankowski and Mungham 1976, Haas and Shaffir 1977, Lacey 1977, Atkinson 1981, Paterson 1983). The education of business and commercial professional groups have been the subject of more recent critical sociological scrutiny (Parry 1988, Greed 1991, Roslender 1992, Coffey 1993). Educational policy and school processes have also been the focus of recent attention from social researchers, keen to interpret the views of the influential and elite within different educational arenas.

Researching powerful groups and powerful organizational settings involves the researcher entering into a power relationship with the researched. As we are all too aware, the balance of power within the research environment can influence all facets of the research process. Cassell (1988) argues, for example, that access to undertake research can be problematic when social researchers wish to study those in positions of power. Buchanan, Boddy and McCalman (1988) suggest that the language of the research proposal is problematized when trying to conduct research with professional groups or in

formal organizational settings. The terms 'research' and 'interview' can carry negative connotations - of lengthy questionnaires; difficult questions combining 'the obscure and boring with the intimate and threatening'. Similarly the term 'publish' can imply the disclosure of commercial and professional secrets to the popular press.

The balance of the power relationship within the research setting can, then, affect the research and the discourses of the research. These may include: gaining access to research site and subjects; the language used during the project and to describe the findings; the conduct and overall progress of the research; subsequent opportunities for dissemination and publication. Bulmer (1989) suggests that all of these issues may be compounded for the ethnographer, who is particularly dependent upon observation and 'unstructured' interviewing, conversations, social interaction and continued developing field relations.

This balance of power in the research environment is crucial in the practical accomplishment of the social research project. Further, the discourse of power can reach beyond the practicalities of the research project - to how it is presented and re-presented. Carrying out social research in elite settings, and with elite groups is also about the nature of the narratives, stories and accounts which emerge from the research. The representations of 'realities' are in themselves powerful discourses. The creation of 'text' and the reading of text based on social research is open to interpretation and influence. This paper begins at that juncture, and concentrates on how the 'field' of ethnographic fieldwork is constructed. Construction refers not only to our observances. In the writing, reading and re-writing of text the 'field' is also constructed. Even in elite research settings, the researcher has power over the creation of the text, the field being constituted by the ethnographers own gaze. The researcher's ability to construct a 'text' of the field (Atkinson 1992) has consequences for the knowledge claims we advance through our ethnographic 'stories'. It also has consequences for the 'reading' of that knowledge by researchers and the researched.

The research setting and the research project

This paper draws on ethnographic research conducted in a United Kingdom office of an international firm of chartered accountants. The focus of the research project was the learning experiences of a cohort of recent university graduates, during their first year of working at the accountancy firm. Concerned with the context of situational learning within an organizational setting, the study followed the training programme and simultaneous working experience of the graduates from when they first joined the accountancy firm, until their end of year review. The methodological approach of the study drew upon an interactionistic perspective. The sociological investigation used mainly ethnographic methods of inquiry, particularly those of observation (participant and non-participant) and ethnographic interviewing. Fieldnotes were the primary method employed to record and collate the data (cf. Hammersley and Atkinson 1983, Delamont 1992, Parry 1992).

Fieldnotes were kept in three main ways. Firstly substantive fieldnotes were made. These consisted of descriptive accounts, both of a general nature and of specific events, actions, people, interactions and settings. Secondly I kept a field diary. This began before I entered the field and continued throughout the length of the project. The diary was an account of my own place within the field and also reflected on the methods I was engaged in.

The diary provided an ongoing narrative of how I felt about the research and reflected the 'reflexive' nature of the research process. Thirdly, I constructed analytical memoranda (Burgess 1984). These were made separate from the substantive fieldnotes and demonstrated my on-going attempts to play with and make sense of the data. They recorded emergent themes, concepts and categories and noted choices I made in how I recorded, selected and used the data. Fieldnotes were kept and analysed over the course of the field project.

I conducted initial interviews with key social actors within the organization - other graduates, office tutors, managers and partners - before beginning observation. The observational fieldwork began on the day a new cohort of graduates joined the firm as full-time graduate accountant trainees. In the United Kingdom, chartered accountancy training takes three years to complete from the time a 'student' joins an accountancy firm. During the three years the student takes on a double role. They are engaged in periodic courses and must study for a series of professional examinations (which are held over the course of the three year training contract). At the same time students are working employees of the accountancy firm, carrying out a wide range of (mainly) auditing work and taking on growing organizational responsibilities. By concentrating on the students' first year with the accountancy firm, both training courses and work experience could be observed and explored.

The observational fieldwork was carried out in clearly defined time-periods, agreed at the outset by myself and the accountancy firm. Both during the observational fieldwork and in between those time periods, the graduate accountants and others involved in their training programme were interviewed. The overall aim of the project was to build up a sociological picture of how a group of graduates received, evaluated and responded to professional education and training within a formal organizational setting. The project is therefore located within sociological literature on occupational and organizational socialization (Becker *et al.* 1961, Becker *et al.* 1968, Olesen and Whittaker 1968, Bucher and Stelling 1977, Dingwall 1977, Lacey 1977, Bloom 1979, Atkinson 1981, Atkinson 1983, Dean 1983, Bunton 1985, Hockey 1986, Melia 1987, Fielding 1988, Parry 1988, Power 1991).

Fieldwork and the process of re-entering the field

In my fieldnotes and in the later writings about the project I refer to the accountancy firm by the pseudonym 'Western Ridge'. The fieldwork took place over the course of a year, and included both observational and interview methods at different points in the cycle of the field research. As a means of developing the key theme of this paper, the discourses of power within social research, attention is drawn to a particular stage in the fieldwork. I originally spent two complete months with the accountancy firm, engaging in participant and non-participant observation of the research environment and the graduate accountants' experiences of it. After that period of intensive fieldwork I partially withdrew from the field. In the subsequent months I kept in regular contact with the graduates, meeting them 'socially' and also for more formal pre-arranged interviews. It had always been agreed between myself and 'Western Ridge' that I should return to the field site for a second phase of observational fieldwork.

I re-entered the field almost seven months since the last intensive period of observational fieldwork. While I had maintained regular and prolonged

contact with the graduates, re-entering the field to undertake further observational study afforded me the opportunity to view how the graduates had adapted to life with the accountancy firm, and to engage in a series of informal 'catch up' conversations with both the graduates and other key social actors involved in the training process.

Buchanan, Boddy and McCalman (1988:65) argue that 'getting back to continue research is not necessarily simple or automatic'. They go on to discuss the potential need to re-negotiate access and renew field relations. As 'going-back' had always been a part of my initial arrangement with the firm, the actual process of re-entering was relatively unproblematic. What was far more problematic was the ways in which re-entering the field made me aware of the socially constructed nature of the 'field' of my 'fieldwork'. Between the two periods of observational fieldwork I had been engaged in reading and re-reading my field notes, analysing and writing about the data I had gathered. Going back, having read about and written about the field, was strange and to some extent disorientating. This is reflected in the entry in my field diary on the day I returned to the fieldsite.

I suddenly realised today how much the ethnographer 're-invents' and socially constructs that which they knowingly call 'the field'. Sitting back in the once familiar seat at the back of the training room I was struck by the fact that there was no field... When I 'lived' with Western Ridge for all those weeks I served as a sieve for all the possible data. I selected the incidents to record, the conversations to note or copy down verbatim. As the fieldwork progressed I worked with a number of different themes and ideas. These were undoubtedly different from what the graduates, the tutor, the accountancy partners probably considered important or significant. Another ethnographer would also have probably gathered very different data. I've now spent several months writing about 'Western Ridge'. Writing about one's fieldwork is rather like writing a novel. Western Ridge is almost 'my firm'. To the extent that I have constructed it from my data, it's fictional. Similarly, the graduates - Bob, Tom, Susan and so on are characters in the 'novel' not real people. I have only captured what I chose to capture. Similarly the tutor, Rachel in my story, is not quite the same as the 'real' person.

Returning to the accountancy firm and re-engaging in intensive observation was accompanied by a realization on my part that I had constructed or created 'the field' which I had been observing and writing about. This demonstrated to me that I had considerable power over what I recorded, analysed and wrote. At the same time I found myself caught, at the boundaries between the field itself and my construction of it in my observations and writing. Going back, I was confronted by the 'real' firm and the 'real' people, and found that they did not quite match the 'field' I had spent so long writing about. My version of events, characters and settings had, to some extent, drifted from what I was now observing. I found myself observing the graduates and thinking to myself that they were different to the characters I had been analysing and writing about. It was almost as though Western Ridge and the characters within it, had taken on separate and distinct entities from the 'real' firm and the 'real' people.

The notion of constructing an ethnographic text, and in doing so 'creating and peopling a world' is of reference here. Krieger (1979) draws comparisons between the sociologist and the novelist. Krieger suggests that both sociologists and novelists need to construct and develop a plausible reality complete with living, 'real' characters. The graduate accountants which I had written fieldnotes about and stored information about in my head had become characters in the plausible reality of 'Western Ridge'. The combination of my fieldnotes and 'head-notes' (Ottenberg 1990) had enabled me to 'people' the world of the accountancy firm. As an author it had seemed natural to 'allow' characters to develop as the story progressed.

Tom provides an example of this process of peopling a social world. Tom was an graduate at Western Ridge who, from early on, came over as arrogant, not only to me but also to other graduates and staff I talked to. His character developed as an arrogant, but smart 'player' in my story, often witty but also difficult to work with. Unconsciously, or subconsciously, these elements of the character became stronger and more pronounced as the construction of my ethnographic text progressed. Other characters, I observed, developed in similar ways. For instance Richard had been noted by his colleagues and seniors as someone who tried extremely hard in his work, but who found much of that work difficult. From the beginning of his training contract he had demanded a considerable amount of time from tutors and fellow students. In my writings his inability to understand and his label as an office 'pain' had become the central tenets of his 'character'. A further example is the character I called Elizabeth. Elizabeth stood out as a student who did not really fit in with her fellow trainees. Her colleagues described her to me in terms like 'a nice girl', 'not ruthless enough', 'not a go-getter'. She found it extremely difficult to make her mark in classes and found the audit work difficult. She also fared badly in her first examinations with Western Ridge. In the text she comes over as feminine, 'girlie', ineffectual and not cut out for the business world. Again certain characteristics and features of her had been emphasized and built upon.

My initial difficulty with re-entering the field was, therefore, reconciling the 'field' I had constructed (and the characters which peopled it) with the accountancy firm as a 'real-life' organization. While I had not set out to purposely distort what I had observed, I had become acutely aware of my role, not only as observer, but also as selective filter, notetaker, reader and author. I had used my fieldnotes and my power as author to create characters and a social world which gave the story interest and meaning. Returning to the field setting raised my consciousness of the meanings and understandings the researcher imposes on 'the field'. Further, those meanings and understandings give the field a life of its own, making it almost a being or entity in itself.

Fieldnotes and the Production of the Field

The realization that the field is a social and a textual creation of the researcher as 'ethnographic observer and writer' is demonstrative of the highly situated nature of ethnographic description (Geertz 1988). It is also illustrative of the authorial presence within the ethnographic processes of notetaking, writing, reading and telling. As Atkinson (1990, 1992) reminds us, 'the field' is not something 'out there' that can (or should?) be recorded completely, accurately or neutrally. Rather fields of fieldwork are constituted in the ways in which we remember, record and retell. Hastrup (1992) argues, therefore, that the reality

of the field is of a peculiar nature, involving the ethnographer not only as observer but also as actor, author and teller.

The construction and 'creation' of fieldnotes (Jackson 1990) helps to produce the researcher's 'field' of fieldwork. As Wolf (1992) argues fieldnotes are in themselves text, giving a 'frozen' description of actuality. The processes of remembering, recording and writing about the data we collect in the field, places the ethnographer in a position of power. Rather than simply being a neutral teller of reality, the ethnographer takes on the role as author, constructing and creating a story or an account of what they observe (Geertz 1988, Hastrup 1992). Fieldnotes and the subsequent telling of the field, thus, depend upon how the social researcher as author constructs conversations, actions and events into a narrative and descriptive form. As my experience with the accountancy firm demonstrated to me, fieldnotes are not simply factual notes, or the recording of 'all the many and varied observable and memorable sayings and doings' (Atkinson 1992:17). Rather fieldnotes are in themselves socially constructed text, developed and created by a authorial figure. The author at the same time has their own unique place in the proceedings, bringing to the creation of the account their own meanings.

Fieldnotes then, are not 'raw' data. They are analysed and interpreted by the author as they are written and read. Fieldnotes come encoded with the authors conscience, understandings and interpretations. As Hastrup (1992) argues, the ethnographer's conscience and presence is part of the reality of the field. As author, the ethnographer sifts, solves, analyses, remembers, writes and in doing so would find it impossible to eliminate their own consciousness from the activities they observe and record in the field. Through going back into the 'field' site of the accountancy firm I became conscious of the process of taking and making fieldnotes, and how in doing so I was involved in constructing and producing textual representations of the social reality of the accountancy firm. Further the processes of writing, reading and rewriting were pertinent in the reconstruction of a particular version or versions of a social world - the accountancy firm Western Ridge.

Western Ridge as a 'field' was constituted by what I chose to record and how I chose to record it; what I wrote, read and rewrote about. The 'reality' of the field was circumstantial to my presence as ethnographer (Hastrup 1992). The field only existed in its particular form in what I had written and what may be read. Fieldnotes then, as ethnographic texts are constructed (Geertz 1988). The data of ethnographic work are themselves textual products. Fieldwork then, it follows, is about a practical activity and a literary activity. The field is constituted not only by our ethnographic gaze, but also by our construction of a text of what is 'gazed' and our interpretation of what we write (Atkinson 1992). As my experience of re-entering the research site demonstrated to me, the textual representation of the field, fieldnotes and other readings and writings, define the cultural and social boundaries of that place we 'call the field'.

It is initially through the taking and creating of fieldnotes (Jackson 1990) that we constitute the field and its vocabulary. The power of the ethnographer as author of fieldnotes, and of subsequent texts, draws our attention to how, within a fieldsetting, the ethnographer has power over a certain type of discourse. While the social researcher may be involved in researching a perceived powerful organization or group of people, the ethnographer has power over what is collected, interpreted and written. This is demonstrative of by Krieger's (1984) comparison of sociologist and novelist.

Ethnography, Suspense and the Making of an Ending

The move toward an ethnographic approach which is reflexive, interpretive and literary (Bretell 1993) is one to which many contemporary ethnographers have moved (e.g. Marcus and Cushman 1982, Krieger 1984, Clifford and Marcus 1986). The recognition or notion that the ethnographer is a creator as well as a writer is a powerful one. The creation of a text of the field suggests that as well as author, writer and reader, the ethnographer is also a storyteller. As a narrator of the social world they have observed and taken note of, the ethnographer is responsible for the creation of a 'readable', and arguably interesting text or story.

The social scientist as ethnographer is not unlike the novelist. Both desire to give a view of a social world which is interesting and 'readable'. As Krieger (1984) argues, social scientists often turn to the realm of the novelist; invention, illusion, inner vision, a focus on the unique and peculiar. The 'sociological imagination' may incorporate or take on board what Krieger (1984) calls the 'fiction temptation'. As novelists provide a version of reality, a model of the social world, so then do social scientists. While the persuasive rhetorics and 'data' may be different, imagination and interpretation serve as tools of both. An example of this may clarify the argument. The example again draws upon my own re-entry into the field, and the collection of new data during that period of fieldwork.

The dilemma of how to finish a piece of academic work and conclude ethnographic writing has not been the subject of much critical attention. Wolcott (1990) errs on the side of caution, arguing it is not necessary to end ethnographic work by pushing a canoe out in the sunset. Delamont (1992:183) makes the comparison between the fictional fairy story and qualitative research.

In fairy stories, the conventional sentence of closure is 'they all lived happily ever after', but life is not like that, and it would be a peculiar piece of qualitative research which ended with that stock conclusion.

At the time at which I re-entered the field site of the accountancy firm, I was prepared for a 'stock conclusion'. That is, the student accountants had reached the end of their first year of accountancy training and were preparing to begin their second year. My initial intention when I first embarked on the project was to explore how university graduates 'became' part of an elite and powerful business 'profession'. 'Training for success' had been an early working title I had played around with. Over the course of their first year with the firm, some of the graduates had not found this transition easy or even possible. One graduate had already left the firm by the time I returned for the second stage of observational fieldwork, and I was aware that others had experienced difficulties with both the work and the studying aspects of their training contract. However, from meetings with the firm and previous students before I even began the fieldwork, I had been led to believe that it was unusual for students to fail, and that for the most part promotion at the end of the first year was a matter of routine.

It became increasingly clear when I re-entered the field that this 'routine' was far from automatic for this particular group of students. The students were far from happy with their work experience. More importantly perhaps, the accountancy firm were withholding promotion for some students as they were unhappy with their standard of work. What I had foreseen as a 'neat'

way of catching up with students one year on, and a way of providing a 'conservative closing statement' (Wolcott 1990) had been challenged by the fundamental change in the students' circumstances. The return to the fieldsite gave me access to very difficult circumstances and data, from my earlier visits. The process by which I 'managed' the collection and writing up of this 'new data' transposes the 'fictional temptation' of the social scientist.

By the time I re-entered the field I had already written up much of the project, based on the earlier protracted fieldwork and ethnographic interviews. My original intention was to add to and update chapters, incorporating the students' views one year on. It became clear that the route to professional accountancy status was turning into a rather bumpy one. As the students spoke to me of their disillusionment, it was apparent to me as author that I was being 'given' a dramatic and not altogether happy ending to the students' story. I went back to my earlier fieldnotes and to all of my chapters and writings I had drafted. While I could see anxiety, worry, even scepticism in the students' early training experiences, the tidal turn in their fortunes, and in how the firm received their work was not evident. If it had been present earlier, then my observation and interviewing had not gleaned it. Up until my re-entry into the field the story was one of students trying to get on, learning the ropes, finding their feet, competing with each other. It was a story of discovery, of anxiousness but also largely one of hope. There was never really any sense that they were not (on the whole) going to make it.

The prospect that some of the graduates would not receive the 'automatic' end of year promotion, and as a result may choose to leave the firm and accountancy behind, gave a different ending to my ethnographic 'tale'. As I became clearer of this (unexpected) ending I was faced with a methodological and literary decision, as the narrator of the students' story. It was not really a case of deciding how to tell the story. I was committed to 'giving voice' to the students, of telling their story (Richardson 1990). In constructing my 'ethnographer's tale' I had tried to take events and actions, and order them into thematic temporarily meaningful episodes (Polkinghorne 1988). What was at issue was not the telling, but where to tell. Data collected from when I re-entered the field needed to fit in with the rest of the story. The thesis needed to flow. I needed to maintain fluidity and a sense of chronology. But I also needed to signpost the change of circumstances and give the students 'voice' to understand those changes. On top of that I was excited by the dramatic events and by the possibility of challenging the expected ending. The solution which I decided to adopt reinforced the narrative and the fictional within the telling of the ethnographic tale. My desire to maintain a sense of a complete and rounded 'story' combined with the need to incorporate the changes in the graduates' perceptions of reality, led me to use the new data to create the essence of a good story - a good ending.

I decided not to alter, in any fundamental way, my earlier writing and story-telling. I wrote up the changes in the students' circumstances and in their perceptions as a narrative in itself. I outlined how the 'clouds had gathered', how the firm had indicated to the students that things were not going 'as expected' and how the students were interpreting all of this. I then used this narrative to form the 'ending with a twist'. As it was an unexpected ending I placed it at the end of the thesis as the last chapter. In the preceding chapters I did not make direct reference to the unexpected. Where the chapters related to issues which later re-emerged as important to the students in interpreting their changing circumstances, I suggested as such. Phrases like 'this is further explored', 'this view changed'; 'circumstances changed later

in the year'; and so on were woven into the text, thus creating a sense that there was more to come, and that it may be different from the expected.

Krieger (1984:269) talks of the fictional temptation in ethnography, and suggests that 'we can expand our options for writing up social science field research by using methods of fiction'. What I did was to scatter the thesis with temptations of more, grab the readers' attention, give a feeling of suspense, of an ending in store without actually giving the ending away until the last chapter. The thesis remains optimistic and hopeful, until the end, or almost the end.

Creating a sense of suspense in order to prompt the reader to want to go on, is the stuff of the thriller, the crime story. I am always struck by how confronted I am when I get to the end of a novel and the ending is different from the one I was expecting. These stories which end 'happily ever after' (or at least as expected) are the ones which one may enjoy but be left a little disappointed with. On the other hand, those sort of endings are safe and on the whole do work. The twist in the tale is not so safe, but is the stuff good novels are often made of. The success of the creation of suspense is, of course, in the reading as well as in the telling. I was aware at the time that my ethnographic story was to be read by an academic audience, in the first instance by two distinguished professors. One confessed to quickly skipping to the end to find out 'what happened'. The other read the thesis from cover to cover, not wanting to spoil the ending...

Authorship and 'Authority'

Ethnography is about 'authorship'. The textual representations which we create contain our power as author to turn 'a field' into a readable text, 'a narrative construction of a social world in a readable textual form' (Richardson 1990). However, this process of textual creativity is not just portraying a version of the 'field' for the author (as ethnographer) is also part of the version. My authorial presence was present in the texts I was producing. The 'knowledge' I was writing was bound up with my own social interactions within the research site. Hastrup (1992) describes this as the confrontational knowledge of the ethnographer. Jackson's paper on anthropologists' fieldnotes (1990) highlights this phenomenon, recognizing that while ethnographers create fieldnotes, fieldnotes also create and maintain the anthropologist's own identity. While fieldnotes are 'data' and a record of the field, they are also the 'ethnographer'. Moreover, this is an inherent part of the ethnographic enterprise (Grazier 1993). As Wengle (1988) has argued, while the ethnographer desires to be the fly on the wall; an unobserved observer able to come away with the 'real' story, the ethnographic text also constitutes a cry for recognition of being there, or 'I was there' (Grazier 1993).

The field is not a naturally occurring category or site, existing because the researcher as author produces it. The researcher as author is also involved in producing themselves. Any effort to write ethnography requires the anthropologist/sociologist to return repeatedly to the circumstances, events and moods of one's fieldwork. We re-read fieldnotes, transcribe tapes, pour over diaries that reveal more about ourselves than our research (Sheehan, 1993:78). Re-reading my own text revealed to me that the boundaries of the field were tied up with my own social transactions and 'confrontations' within the field. I was actually engaged with the text, not only as observer but also as actor and as author. As such my fieldnotes documented and told a 'reality' of the field and also represented in textual format myself, my relationships and

transactions. Fieldnotes, in this case, provide memories, and a record of what 'the field was like' but also records the cultural relationship to the field. I use the experience of 'going back' to explore how ethnographers, as writers and authors socially construct a field, of which they themselves are de facto a part.

'Entering the field' or 'going back into the field' are both part of the vocabulary we use to constitute and give meaning to the sites and subjects we chose to research. Atkinson (1992) describes the field as a clearing hewn out of the social forest by the fieldworker's own acts of access, the boundaries of which are tied to the fieldworker's own social memory and transactions. The field is also created through the ways in which we choose to write and read about what we see. When I re-entered the field I actually found observation difficult because I was conscious of whether I was telling it like it was, as 'a real account', or whether I was constructing story or tale which was somehow not the real story.

The above argument sounds as though the power of the ethnographer as author is somehow sacrosanct. We take fieldnotes on the social worlds we are seeking to understand, we analyse write, read and rewrite until we have an account which tells the particular story we wish to tell. In reality the power of the author is, of course, mediated by a variety of factors such as our relationship with those we research, our sense of responsibility to our informants, and the corresponding power informants may have over the researcher. Authorship of ethnographic texts is a process, or as Horwitz (1993) argues, a negotiated dialogue among ethnographer, subject and reader. Horwitz writes that informants never have total control and no one is utterly free to choose a narrative (Horwitz 1993:135). Ethnographic storytelling incorporates with it different discourses of power. Authorship is related to, but not necessarily synonymous with, authority. Throughout my own fieldwork, and particularly during the writing up of the data I was conscious of both my own status and that of my informants. Sheehan (1993) describes the multiple personalities of the ethnographer.

I was a needy student and a junior scholar. I was also a member of the British sociological community with overt intentions to publish the results of my ethnographic endeavours. Western Ridge was an international and potentially powerful organization, who had been kind enough to give me access but also had the power to prevent publication (through legal means if necessary) of perceived sensitive information. The graduates within Western Ridge were very junior members of the firm. They had little current status or prestige, and I was acutely aware that in telling their story I had to be sensitive to their ambiguous position with the firm. It was after all, their careers and their impressions/experiences of the firm which I was trying to track.

During the collection of data, and the writing up of that data, I was aware of making decisions. I have already referred to that at an earlier stage in this paper. However alongside deciding how characters should be shaped and how the text could be made a readable one, there also remained the issue of what would be 'safe' to write. I did not want to produce anything that would offend my informants or their seniors. Nor did I wish to 'make life difficult' for the students. As an academic my responsibility was not only with my informants, but also with the scholarly community in which I wished to work. For all of those reasons there was a tension between what I learnt and what I felt able to tell. It is not that I 'discovered' huge scandals or questionable practices. Rather I faced the problem of 'making ethnographically meaningful connections between publicly and privately available information' (Sheehan 1993:80).

This had to be done without jeopardizing my own professional integrity, the graduates' careers or my relationships with the fieldsite. This included deciding not to write about some events, and using strategies to ensure anonymity. Quotes from informants were given an active role in the construction of the text, but I used different pseudonyms for different quotes from the same individual if that seemed appropriate. I also worked on some of my characters taking on composite forms where information and characteristics were peripheral to the sense of the text. In her study of menopausal women in Newfoundland fishing village, Davis (1993:31) describes similar anonymising strategies, disguising the 'identity of individual women as far as my standards of truth would allow'.

After my thesis had been written I presented a copy to Western Ridge, partly because I felt a moral imperative that they should be able to see what I had written, and partly because I wanted to give them a chance to respond and discuss any pertinent issues. It was understood that I was happy for the senior members of Western Ridge and the graduates to read the text. I was reasonably confident that it gave a balanced account of my observances. The graduates had seen some of the chapters at draft stage and had 'approved' my overall interpretation of key events and their responses. Bretell (1993) draws to our attention her observation that when discussing the relationship between readers and writer, native readers are often not considered. By that she refers to informants and others who have a vested interest in the ethnographic text. 'When they read what we write' can be a test of the authority of the author and the power balance between ethnography and the field.

Despite my care to anonymize the fieldsite and social actors within it, the accountancy firm objected that in their reading of the text they could identify both the organization, and the narrative and descriptions of individuals within it. While I had given the firm a pseudonym and disguised any particular identifying factors they maintained that it was still possible to identify them. This is a telling objection, given that my initial fieldwork access negotiations had included the firm's desire to glean 'publicity' from my project by maintaining their identity, and my insistence at that stage that ethically I could not agree to doing so. Powerful individuals with the accountancy firm had read my account and made it clear to me that they were interested in discovering who had told me what. Further, they were clearly unhappy with some of my descriptions of events and how the graduates had responded to them. The firm had even gone as far as asking individual graduates to attribute particular quotes, which they had declined to do.

The authority of my text as a realistic account of what had happened during the graduates' first year with Western Ridge was questioned. My own academic integrity came under detailed scrutiny by the firm and was found wanting. It is also worth mentioning here that the text to which they laid so much scepticism and dislike was a sociological thesis written with an academic audience in mind. In being so, it drew upon sociological and anthropological literature from a wide variety of settings, not all of them professional or business organizations. While the firm recognised that the thesis was already in the public domain they requested that I agree in writing to decline from publishing some aspects of the text which they found particularly sensitive or basically wrong. This again alludes to the dynamic nature of the power discourse in field research and ethnographic writing.

Beyond the bounds of the project, in the wider academic and publishing community, the accountancy firm felt I had potential power over them, through

my ability to publish about them. Simultaneously my scholastic integrity and my responsibility to the firm meant they had the power to prevent me from publishing particular aspects to which they objected. Of course, the complexity of the power dynamic goes far beyond those aspects. It is not inconceivable that Western Ridge as a powerful and financially buoyant organization could take formal proceedings to prevent my publication (but with the cost of making the 'sensitive' areas public in other ways). It is also the case that my biggest responsibility was to my key informants, the young graduate accountants. I was acutely aware that any resistance on my part could result in their professional lives and careers being made extremely difficult by the organization at which they were employed. If part of the ethnographic enterprise is to take people's own words away from them and reconstruct them for our own purposes (Davis 1993), then another part of the same enterprise must surely be to remain responsible for how those words are then interpreted by those who read them.

Conclusion

This paper has sought to demonstrate how the accounts we write and read of our ethnographic enterprises are shaped by discourses of power. The arguments put forward here are not exclusively relevant to the research of elite groups and organizations. The power we have as author over minority or underprivileged groups is acute and should be recognised as such. In reading about what we write these groups may not like it, but may have little power to exert in preventing us from wider dissemination. However even in such cases, as Davis (1993) observes, individuals and communities can make life very difficult for further research and friendship if they do not like what they read. In the case of powerful groups, how to write about them also takes on significant meaning. While a powerful research setting can leave an (especially junior) researcher feeling victimised and powerless by their criticism and exclusivity, the researcher retains power as writer and author. That power, however, is not fixed or permanent and is subject to reduction by the research population at a later stage.

I was in the position of studying a relatively powerless group (but longitudinally potentially powerful) within the context of an elite organization. As such the discourse of power was further compounded. I had the power and responsibility to give voice to the young graduates and also the potential to create severe problems in their professional lives and accountancy careers. Studying the powerless in a powerful organization has its own dilemmas after the use and abuse of power.

To conclude, I have argued that textual representations transform social actions and events into narrative, which in turn helps to shape and give consequence to the details of observed life. However, the narratives and representations the ethnographer crafts should not be viewed as 'fictions' simply because they are 'made' (Atkinson 1992). By placing the observable into recognizable textual formats, the ethnographer can make the social word readable. The power of the ethnographer to do this should not be taken lightly, nor should that power be seen as given. Rather it is part of complex discourses of authorship, authority and responsibility.

If ethnography is a negotiated dialogue among ethnographers, subjects and readers then perceptions of power form part of that dialogue. Attention to the ways in which researchers represent, construct and reconstruct the 'field' (and themselves) is a constituent part of the process of reflecting and

encapsulating the social world we seek to understand. Attention to the ethnographer as author and to the informants as readers also forms part of our understanding of how our accounts can be powerful and may be used as tools by the powerful.

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